contrary to the then generally accepted view, were not substances necessary to the growth and life of every cell, but had a specific action on certain tissues resembling that of hormones. As Sharpey-Schafer had coined the word "autacoid" for substances found in the body and having a drug-like action, I suggested the name "trophacoid" for analogous substances present in the food. The name matters little. What matters is the clear recognition of the fact that the diseases resulting from a deficient supply of vitamins are not diseases *sui generis*, but fall into line with those due to endocrine dysfunctions. Like the latter, they are the result of specific pathological changes in certain tissues, the changes varying in degree with the degree of deficiency in a particular vitamin or combination of vitamins, and the length of time over which this deficiency is operative. These deviations from the normal can sometimes be restored by supplying the vitamin which has been lacking, in the same way as the changes resulting from endocrine dysfunction can be restored by the requisite hormone. But with some of the vitamins, especially A and B, these primary pathological changes, if maintained long enough, lead automatically to the development of other pathological conditions, which are thus an indirect effect of the vitamin deficiency, and which, therefore, do not disappear when the vitamin is supplied. An understanding of this relationship is important in assessing the therapeutic possibilities of vitamins. It is even more important in showing that the failure of a vitamin to "cure" a pathological condition does not necessarily prove that there is no aetiological relation between that condition and the deficient vitamin supply. Such a simplification between cause and effect holds good for those pathological conditions which represent a primary effect, but not for those where a secondary pathological change has developed on the basis of the primary one.

A deficient supply of vitamin A produces as one of its primary effects an atrophy of the mucous membranes of the respiratory and of the digestive tracts,[1] and of the mucous glands, such as the para-ocular glands. This enables the rather avirulent bacteria normally in contact with these tissues, and kept in check by their secretions, to infect first these tissues, to penetrate from there into the blood stream, and to set up metastatic infections elsewhere, although the effectiveness of the humoral defences is not diminished by the lack of vitamin A.[2] These findings have been amply confirmed since. I may point out in passing that the evidence available at present does not seem to me to justify the extension of this change in the epithelium of the mucous membranes to all epithelium, as Mellanby suggests. The xerophthalmia, for instance, is due to the atrophic change induced in the para-ocular glands, and not to a weakening in the cornea.

Vitamin A may be expected to restore the atrophic condition of the mucous membranes, but it cannot be expected to act upon the metastatic infections, which originated from the infected mucous membranes. Under experimental conditions it does not even free the mucous membrane of the respiratory tract from its infected conditions. Young rats which have been submitted to a vitamin A deficient regime sufficiently long, and are then put back on a vitamin A diet, will resume their growth and live for a year or more. But a large number of them will still show heavily infected lungs. The absence of vitamin B produces effects similar to those of vitamin A, if it is continued over a long period by allowing the animals to recover from a B-deficient diet and repeating this procedure many times. In such an experiment I have seen even xerophthalmia develop on a diet which was free from vitamin B but contained cod-liver oil. This is due probably to the defective absorption of food in the absence of vitamin B.[1]

It was on grounds such as these that I opposed Mellanby and Green's conception of vitamin A as a generally anti-infective agent. I pointed out then (*Lancet*, 1930, i, 1153) that the exaggerated claims made for vitamin A would be followed by disappointment, which would discredit the more limited though really effective action of this vitamin. It is a melancholy satisfaction to be able to say "I told you so," but some recent writings appear to justify my forecast. If this came to pass it would be deplorable. For the extent to which an adequate supply of vitamin A and B can prevent a variety of chronic infective conditions is still not realized. That this aetiological relation is not yet recognized is due to the fact that, for the reasons given above, these conditions, when once established, cannot be cured by an adequate supply of vitamins. The proof depends on the less spectacular but more rational evidence of preventing them. Any doubt on this point should have been allayed by the large-scale experiments carried out recently by R. McCarrison (*British Medical Journal*, 1931, i, 966). This experiment also shows once more that in order to maintain an organism in perfect health it is not necessary to add preparations especially rich in vitamins if the diet is built up around a sufficient amount of milk, butter, and bread which has not been deprived of its vitamin B content.—I am, etc.,

Imperial Cancer Research Fund, Dec. 5th. W. CRAMER.

[1] Cramer, W.: *Lancet*, 1923, i, 1046; ibid., 1924, i, 633.
[2] Cramer, W., and Kingsbury, A. N.: *Brit. Journ. Exper. Path.*, 1924, v, 300.

"How Do Drugs Act?"

SIR,—All medical men interested in Hahnemann's teaching will be gratified by Professor Langdon Brown's reference to the value of Hahnemann's contribution to medicine. But his followers are not prepared to admit that wisdom perished with him, or that no further "progressive conceptions" have emanated from the homoeopathic body since his time.

The Regius Professor of Physic rather unreasonably expects a body of clinical pharmacologists—for that is what homoeopaths are—to contribute to the sister sciences of pathology and diagnosis, and claims that they have failed to do so. But in the course of their work they have produced a considerable body of research which has already proved of value to medical men in general. I would particularly refer to Bach and Wheeler's prolonged researches into the role of the non-lactose-fermenting organisms in maintaining chronic disease.[2] Again, attention should be drawn to Paterson's work on bacterial dissociation.[3] In the field of physiology Boyd's work on the periodic fluctuations of the electric field of the human body is an important contribution.[4] As for apparatus, Boyd's emanometer must not be forgotten. True, this is still an instrument of the research laboratory, but its inventor has devised many refinements of technique since his striking results were investigated by Lord Horder's Committee and published (in 1924). This is an instrument of diagnosis—and diagnosis of a refined degree. It is not so much a matter of determining a label, a disease-name, as an investigation of the patient's idiosyncrasies. It is here that the outlook of homoeopathy departs from the orthodox and approaches that of the neo-Hippocratist, or perhaps the "return to Aesculapius," already commended by Professor Langdon Brown.[5] The homoeopath attempts to diagnose the patient's "reactions as an individual" (the professor's own phrase), and it is the

[1] Mottram, J. C., Cramer, W., and Drew, A. H.: *Brit. Journ. Exper. Path.*, 1922, iii, 179.
[2] *Chronic Disease*, London, 1925.
[3] *British Homoeopathic Journal*, July, 1933.
[4] *Brit. Journ. Radiology*, April, 1932.
[5] Individual Psychology Pamphlet No. 4.

diagnosis of the patient's specific pharmacological idiosyncrasy that leads to the successful prescription. This stone, which Professor Langdon Brown neglected in his address, is indeed the "head of the corner" for the modern homoeopath.

All this, Sir, should go to prove that the homoeopathic ideal, far from being crystallized out, is still a supersaturated solution, from which we may expect in the future fresh showers of many-faceted crystals.—I am, etc.,

Bristol, Dec. 5th. FRANK BODMAN.

SIR,—Wishing to enlarge my knowledge of "how drugs act" I have gone carefully through a great part of the homoeopathic literature during the last two years, and on the basis of that work I cannot agree with Professor Langdon Brown on the sterility of homoeopathic work in pathology and diagnosis. The work of the homoeopaths has given us important experimental research into the action of drugs on healthy individuals which affords a far deeper insight into drug action than animal experimentation. The homoeopaths have given also to diagnosis an extraordinarily accurate study of symptoms and the most precise method of the diagnosis of the individual patient. It would have spared me many errors and much groping in the dark if I had known of the work of Hahnemann and his disciples earlier in my career.

The place of homoeotherapy in contemporary medicine is very well defined. On the basis of a careful diagnosis of the disease as well as of the patient the physician has to choose his remedy from three great groups, the aetiological medicines, such as arsenic for syphilis, the antipathic medicines such as laxatives for constipation, and the homoeopathic medicines. He will be guided in his choice by the results of clinical experience, the only universal law that can be accepted in medicine. This mode of handling cannot be called eclecticism, but must be considered as action according to a definite principle—the Hippocratic principle that "there are conditions which are better treated by similars and others better treated by contraries."

Thus no modern teacher of therapeutics has the right any longer to be ignorant of homoeotherapy. No modern physician has the right to withhold from his patients the benefits of homoeotherapy when this is indicated. It all comes down to the question of efficiency. The physician who is master of these three groups of remedies is a better healer than he who confines himself to those listed in our school materia medicas, or, on the other hand, than one who chooses exclusively from the homoeopathic materia medica. Reform in the teaching of therapeutics on that point is the more necessary in that I am under the impression that, if we except the few diseases for which we have potent aetiological medicines, most morbid conditions are better treated by similars. The old antipathic methods of the medicine bottle and pharmaceutical proprietary preparations seem to have a very limited sphere of action.—I am, etc.,

London, W.1, Dec. 11th. A. P. CAWADIAS.

*** We regret that we cannot find space for a number of other letters discussing this particular feature of Professor Langdon Brown's address.—Ed., *B.M.J.*

Treatment of Rodent Ulcers

SIR,—Most of those who adopted Dr. Adamson's method of treating rodent ulcers by scraping followed by a single large dose of unfiltered x rays will agree that it is the best treatment from every point of view. Except where the age or whim of the patient, the site or extent of the lesion, or the involvement of underlying cartilage or bone suggests the use of radium, it should be the routine procedure. If the scraping is efficiently performed it is seldom necessary to give more than a four-pastille dose, and sometimes a two-pastille dose is sufficient. Under these conditions the cosmetic result is excellent; indeed, if the lesion has been a small one its original site can often not be determined. This method is also suitable for the many cases of early epithelioma which come within the province of the dermatologist.—I am, etc.,

Leeds, Dec. 9th. JOHN T. INGRAM.

SIR,—In recent correspondence in the *Journal* on the treatment of rodent ulcer no mention appears to have been made of treatment by electro-surgical methods, in favour of which I have abandoned both radium and excision; and judging from several cases I have treated that have been previously x-rayed and had CO_2 snow applied, the diathermy method compares very favourably with any other.

There are two methods available—the monopolar and the bipolar. The former is used in most cases, especially where there is underlying bone—for example, the side of the nose. With this current the surface is sprayed; the eschar is then removed with a sharp spoon. This is repeated until the bed of the ulcer is reached. This is sprayed and the eschar left on. The resulting scar is thin, soft, and has minimum contraction. I recently treated a case of a rodent ulcer at the inner canthus, which had been treated with radium three years previously, recurred as a squamous epithelioma, and had extended on to the lower half of the eyeball. In the bipolar method a loop is used and the surface of the ulcer "spokeshaved" to the requisite depth. A coagulating current is finally turned on and the eschar again allowed to remain and separate spontaneously.

These methods have the following advantages: (1) they can be done under local anaesthesia in the consulting room; (2) there is no after-pain; (3) there is a minimum of scar tissue, due, as Wyeth explained, to regeneration of normal tissue; (4) they are under complete control as to depth affected where there is underlying bone—this is of considerable importance; (5) the treatment can be repeated, and when recurrence occurs there is no alteration in the character of the cells.—I am, etc.,

Bournemouth, Nov. 28th. W. ARCH. MEIN, F.R.C.S., F.R.C.P.Ed.

Primary Thrombosis of Subclavian Vein

SIR,—The following case is of interest as it occurred in a gunner attached to the Royal Garrison Artillery, and although not a rower, his case corroborates the views of Dr. C. H. S. Taylor of Cambridge as to the anatomical cause of this condition (November 4th, p. 818). Dr. Taylor states:

"In rowing there is little abduction of the upper arm. . . . There is, however, a good deal of shoulder play at different parts of the stroke, so that the clavicle, pivoting about the sterno-clavicular joint, goes through a considerable range of movement. At the finish of the rowing stroke the outer end of the clavicle is pulled backwards and downwards, and I would suggest that this action may cause pressure on the subclavian vein as it passes over the first rib, and that such a pinching of the subclavian vein is assisted by the pressure of the backward and downward swinging clavicle against the contracted anterior scalene muscle, combined, as Pearce Gould and Patey suggest, with pressure on the vein by the contracting subclavius muscle."

REPORT OF CASE

Gunner W., a powerfully built man, was employed as mess-waiter at the officers' mess, and one of his duties was to polish the metal ends of the barrels of some small calibre field guns. He was left-handed, and one day I noticed that his left upper arm and forearm were twice the size of the right, with a certain degree of oedema of the back of his hand. The skin was slightly cyanosed, with some cellulitis,